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COLONY OF NATAL.

Horse-Sickness Investigation.

INTRODUCTORY.

BY

H. WATKINS-PITCHFORD, F.R.C.V.S.

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Horse-Sickness Investigation.

INTRODUCTORY.

By H. Watkins-Pitchford, F.R.C.V.S.

A GOOD deal has been written of late about Horsesickness, which, though essentially an African disease, seems by its serious nature to have attracted the attention of scientists in other parts of the world than South Africa.

Some months ago a treatise from the pen of Professor M'Fadyean, of the Royal Veterinary College, was published, and following close upon that appears the report of a lecture delivered by the famous French savant, M. Nocard, dealing with the same subject.

The recent report upon the disease—or rather on the advance made in its prevention—published by the Director of the Cape Bacteriological Institute, will be fresh in the memory.

In reviewing these various reports and papers it seems a little unfortunate that it should be necessary to publish results of work done which are only able to do small service towards the real check of the disease and our knowledge of it. Such reports are necessarily inconclusive or conjectural in character, and do little good besides securing to the worker priority of suggestion or result in limited directions, while the drawback to such premature or immature work is that other workers in the same field are perforce obliged also to adopt the same attitude of importunity, or run the risk of forfeiting the claim of originality and priority, which to the scientific worker means success.

As is known, the Natal Veterinary Department has for the last few years been working towards the elucidation of this knotty problem, and while the difficulties and drawbacks have been many — to instance, rinderpest, other urgent work of a similar nature, great and constant departmental demands, and lately the state of war in which South Africa has been plunged—still the work has gone on steadily as the occasion offered, with the result that much solid and satisfactory work has been accomplished, work of elimination and negation —if the term may be allowed—rather than the positive results often obtainable in less difficult walks of scientific research.

Without attempting to place before the readers of the *Agricultural Journal* a tabulated report of the work done and the conclusions already arrived at—which being to a great degree technical would serve no practical purpose beyond demonstrating the methods and difficulties of such work—it seems possible that a description or detailing of the experiments now in progress, and their object, might result in some furtherance of the work if those interested in the matter would enter, to some extent, into the question and not hesitate to express their views, and particularly give any revelant experiences they may possess.

Such experiences and observations must amount to a great deal in a community of intelligent farmers, who, by reason of the difficulty in obtaining professional assistance, have had for years past to make their own observations and deductions, both as to the cause of the disease and its treatment.

In laying my views, therefore, before the readers of the *Agricultural Journal*, I shall deem it a kindness if any points, either corroborating or discounting these views, are brought to my notice by those whose knowledge of the disease is of long standing, or who have suffered heavily from its ravages.

It has been with much pleasure that I have learned, within the past few days, that my impressions and theories as to the causation of the disease have been entertained also by my colleague, Dr. Theiler, of Pretoria, and I shall, therefore, propose to ask him to give his views of the cause of the disease to the readers of our Natal *Journal* in corroboration of those which are now appearing, as, while they were arrived at quite independently of the Natal work, they are reassuring, inasmuch as they record the observations and theories of a careful and experienced scientific worker in the same field. In a report recently published by a South African worker, having had many years of experience with horsesickness, the statement is made that the disease is not due to the agency of insects, such as the mosquito or other blood-sucking insects, for various reasons adduced, and the

cause is declared to exist elsewhere. It is in support of the view here denounced as untenable that experiments are now, and have been since the commencement of the year, in progress in Natal, and it is in concurrence with this view that Dr. Theiler will also add his testimony in the subsequent numbers of the *Natal Agricultural Journal.*

A brief review of the theories and ideas as to the aetiology, or cause, of South African horse-sickness will appropriately accompany a description of the lines of investigation suggested in a previous issue.

The possible modes of infection are three in number which may be termed:—

(*a*) *Subcutaneous,* or Traumatic (*i.e.,* through abrasions of surface). This heading includes the agency of insects who puncture the skin for the purpose of abstracting blood, or otherwise.

(*b*) *Respiratory,* infecting through the air passages, when in a normal or possibly an abnormal condition.

(*c*) *Ingestive,* where infection takes place through normal or abnormal condition of mucous-membrane or digestive apparatus, and under this head would naturally fall the agency of intestinal parasites. I have included also under this heading (*c*) the possibility of infection through the lachrymal mucous-membrane of the eye.

The above possible modes of infection will, I think, be found to cover all the methods by which the organism can gain access to the system of a horse, and we must endeavour to ascertain by which of these channels a horse becomes naturally infected.

We cannot, however, take it for granted that one and one only of the above modes of infection are concerned in the spread of the disease.

These theories as to the production of the disease are, of course, based upon the assumption that South African horse-sickness is microbic in nature, or due to the agency of a micro-organism which has hitherto eluded discovery. This conclusion seems warranted when we consider the well defined period of incubation, sudden accession of urgent symptoms, and the certainty of infection by means of the injection of a minute dose of virulent matter.

The fact that we are not able to demonstrate the organism does not negative the possibility of the agency of such in the production of the disease, for we have several parallels in bacteriology such as rabies, small-pox, etc., etc., in which we are equally unsuccessful in our search for a casual microbe. Possibly improved optical apparatus may bring these minute bacteria within our sense of sight, but it is more probable, seeing the great efficiency to which lenses are brought in modern years, that we shall have to fall back upon corrobatory evidence by the existence of a specific organism such as may be furnished in the investigation of this disease.

(*a*) *Subcutaneous or traumatic theory.*—The arguments in favour of this mode of infection are numerous and weighty, pointing to the agency of insects rather than to simple traumatisms or wounds being the points of ingress of the disease. These later must be very varying both as to place and manner, and the fact exists that no connection has been traced between cases of severe traumatism or wound and instances of the disease (such as obtains in the disease Tetanus).

The only argument tending to confirm the simple traumatic theory seems to be the prevalence of cases as reported by some observers immediately after the early frosts and before the weather has become so cold at night as to be inhibitive to bacterial life. In this case it has been suggested that the act of feeding on frosted pasture being followed by an increase in the number of cases might indicate that the cause usually lay in some laceration of the tongue or lips and the introduction of the disease consequently through these channels of the mouth and tongue, and the incidence of cases about this time might be held to be related to such a cause, and indicate a possible clue to the manner of general infection.

The fact that animals receiving cut and carefully gathered food, in whom no such lesions presumably exist, suffer from the disease, and also that the disease attains its maximum violence at periods free from frost, would tend to discount such a theory. Such a cause in any case could only be operable in a very small percentage of cases of the disease, and its significance is accordingly not great.

The Insect Theory.—If the agency of insects as disease-carriers is admitted, no fact which has occurred to me in considering horse-sickness exists to exclude the possibility of the principle being applied in this disease. Presumptive evidence—where all theories are at present presumptive or tentative—points to the possibility of such an agency in the case of the malady in question.

The points tending to confirm such an hypothesis are many, and parellels are not wanting in the case of such diseases as the malarial fevers of man, filiariasis of the blood of man, both due to the well proved agency of the mosquito. Yellow fever, proved recently to be due to infection by the mosquito, elephantiasis, malaria of sparrows, bats, etc. (furnishing an instance of the infection of the lower animals by the mosquito) the pyrosomal disease (red-water) due to the tick, and Ngana or Tsetse-fly disease, and the various instances brought forward from

time to time of the spread of disorders by the puncture of fleas, bugs, all instancing the power of insects to produce disease in and transfer the disease from man to man, some probably only mechanically as intermediary bearers without alteration of life cycle, and others as definitive hosts in which the specific organism undergoes a phase of development in the body of the insect before it is capable of becoming infectious to man.

In consideration of this heading the weight of evidence in the production of horse-sickness seems to be in favour of inoculation, by the puncture of some insect whose habits closely conform to those of the culicidae or mosquito family. Arguments in favour of this theory are reconcilable with the existing ideas, experiences, and folklore of those having a practical experience of the disease for many years, although the significance of the agency of insects seems to have been entirely overlooked.

The *Dew* has long been considered to possess some influence in the causation of this disease. Exposure of animals during the hours of dew precipitation is generally looked upon as a dangerous proceeding during the sickly season. We are told to avoid exposure of the animal "while the dew is on the ground." The exclusion of dew by closing all doors and nailing sacking over the windows has often been adopted with success. Troughs and drinking vessels have been covered with a similar object. The above precautions are held by many to be unnecessary during rainy weather, when, of course, no dew is precipitated. Heavy falls of dew are particularly to be avoided. A hazy misty morning of early autumn at the close of the rainy season with low-lying mists or fogs is reputed as dangerous until such appearances have been dissipated by the sun.

Such popular ideas as above, connected with the dew and the dew fall, have a significance when it is remembered that the period of greatest activity of the mosquito family is about an hour before and after sundown and one hour before and after sunrise. These periods, therefore, are closely corresponding with the hours of dew precipitation. This idea is somewhat strengthened when one finds some meteorological authorities asserting the existence of two chief precipitations of dew during the twenty-four hours (termed by the French meteorologists the *rosein* and the *serrein*), these two precipitations taking place about sunrise and sunset, and, therefore, corresponding closely with the ascertained periods of greatest activity of the mosquito.

Remembering this fact it was with interest that I heard the testimony of an old Natal Colonist, who narrated to me an experience he had met with when travelling in the Transvaal many years ago. Happening about sundown to find himself belated, he turned aside for the night to a Dutch homestead, but, it being the height of the sickly season, he was somewhat concerned at the prospect of leaving his horse, a valuable one, in strange quarters. On expressing this fear to his host he was told that he need have no fear if he tied his horse in a shed for an hour or two after sundown and then knee-haltered it and allow it to graze, being careful to bring it up again well before the sun rose, and to keep it tied up, if necessary, until the sun was fairly up. This plan, the narrator was told, had been followed for years with unvarying success by his host, who had thus enjoyed an immunity in which his neighbours had not participated.

While there may be considerable truth in the experience, it would occur to people that mosquitos are often troublesome throughout the whole night. It must be remembered, however, that different species conform to different habits of life and hours of activity, and that as many species of mosquitos undoubtedly exist in South Africa we are not justified in discounting entirely such evidence, neither can we be dogmatic upon a subject about which our knowledge is at present to a great degree mere speculation and conjecture.

Such experiences, if those possessing them will take the trouble to record them, may become of considerable value in the difficult work of the investigation of this disease, and I again cordially invite the ideas and theories of those readers of the *Agricultural Journal* whose experience leads them to the formation of an opinion in any direction.

The closing of stable doors and windows would be as effective in excluding winged insects as dew. The covering of water-troughs would possibly prevent the vicinity being infested greatly by mosquitos. That these insects do not generally fly during rain and are numerous towards the autumn after the rains, is a matter of everyday observation, while dampness of grass and humidity of atmosphere seem conditions directly favouring mosquito activity. Thus it is seen that the dew against which we have so sedulously guarded may possibly yet prove the truth of its harmless and poetic character, and be blameable only in so far as it permits nocturnal insects to be its constant companions and close associates.

That the horse is the subject of attack by winged insects needs no demonstration. One recalls such instances as the Gad-flies, the Horse-flies, and various species of the Simulidae or Breeze-flies and the Tsetse-flies, etc., etc.

The well-understood fact that stabled animals suddenly "turned out" are more

subject to the disease than horses which are not stabled or fed, illustrates our own experience that in man a new comer is more liable to mosquito attack on exposure in an infected district than the habitual residents of the locality, and that some obscure protective influence is subsequently more or less rapidly developed, though never probably to the degree of complete immunity.

That this is a tolerance, or adaptation of the host to the parasite, rather than a selectiveness on the part of the insect, seems probable by the reaction to the bite in man generally becoming less and less marked with successive attacks of the insect. That such protective influence is a matter of kind as well as degree seems possible, for instances have been brought forward where a locally established immunity failed to protect in a contiguous district. As an illustration of this point the writer was credibly informed of the case of a resident in the northern part of Natal, who was not troubled by mosquitoes on his own farm—although they abounded there—but who was victimised to a considerable extent whenever he stayed at a friend's farm, distant some twenty miles, and that the owner of this farm, himself exempted from trouble from his own mosquitoes, nevertheless was attacked when visiting his friend's farm.

This experience, strange, even ludicrous, as it may seem, tends to become not altogether incredible as one understands more of the various species, their methods of breeding, habits, etc., some propagating almost solely in rain water pools, others in slowly flowing watercourses and drains, and others, again, in tubs, tanks, cisterns, etc.

It does not, therefore, seem irrational to suppose that a somewhat similar immunity to attack may be acquired by animals not stabled and always exposed, for in this way it seems possible that a protective influence may become established in such animals, enabling them to render inert, by acquired local reaction of tissue or other defensive process not understood, the attack which otherwise would afford a favourable entry for the casual organism.

Such apparent increase of susceptibility in animals suddenly turned out may, of course, be held to be due to alteration of environment, etc., producing in this way a possible "chill" or reduction of resistive power. This action may be argued also to involve risk of producing the disease by other means than that of inoculation by insect-bite.

The disease, in Natal at least, and probably wherever the malady is known occurs at a time of the year when atmospheric conditions are favourable to the activity and spread of the mosquito and winged insect life generally. The heavy rains surcharging the atmosphere with moisture and raising the level of the sub-soil water, form with the alternating excessively warm days of summer an essential and ideal condition for the breeding of the mosquito and allied species of insect, and though the same may be held to be the case with most of the lower forms of life, the constant presence of surface water makes this season of the year one adapted to the propagation of this species of insect in particular.

That no risk is incurred during the actual time of rain seems a wide-spread theory in Natal, and is an important fact when considered in this connection.

Mosquitoes do not fly abroad during rain, though zymotic diseases generally are unchecked by rain. That freedom from risk during rainfall should have been noticed to exist reflects credit on those who, without knowledge of periods of incubation, etc., have made such an observation; which, however, remains to be substantiated.

Cases of horse-sickness have been reported from parts of Natal as having occurred during the winter months, and at a time when the temperature falls during the night to many degrees below zero. This, if the diagnosis is correct, would point to the ability of the mosquito to withstand such extremes of temperature, and not become incapacitated from using the warm hours of midday in order to commit its specific depredations.

That mosquito larvae can withstand the rigours of an Arctic winter is definitely proved, as also that the adult insect can defy the extremes of climates more severe than our own.

The increase of cases with the advent of the early frosts after a bad horse-sickness season seems to point to the possibility of the disease remaining latent in the system of an animal and becoming excited into activity by exposure to low temperature. It is difficult to imagine such causes as those of increased microbic or insect activity giving rise to such cases. The effect of chill or reduced temperature in exciting the malarial symptoms into activity in man is well recognised, and may furnish a clue to this phenomena in the horse.

A matter of more difficult solution under this heading is the question of the Intermediary Bearer. Assuming the active agency of the mosquito in disseminating the disease horse-sickness, the difficulty of establishing the complete life-cycle of the organism, and the identity of the hypothetical intermediary bearer remains, unless the casual organism is simply transferred

from one equine subject to another. It is impossible altogether to exclude the view that perhaps a parallel may exist between the Malaria of man and this disease of the horse, which furnishes several points of possible analogy. The difficulty of establishing the life history of the malarial organism, and the manner in which mankind becomes its victims have been very great, yet in the case of malaria it was at least possible to demonstrate without difficulty the organism after it had once gained an entry into man's system, the chief difficulty being in showing that it depended upon the mosquito for its development and distribution. The maxim of "No mosquito, no malaria," is already passing into a truism.

In horse-sickness, however, we have not the advantage of being able to demonstrate a large organism (more or less filling the blood corpuscle and developing within it), but we have, as I have said above, to take it for granted that a microbe is concerned in the production of this disease, and, working upon that assumption, we have to endeavour to trace its life history unaided by the eye.

It has been noticed for years that mules partially, and donkeys almost entirely, enjoy a high degree of immunity to attack, as well as much insusceptibility to artificial infection. The possibility of their acting as harbourers of the organism without in themselves manifesting physiological disturbance must not be lost sight of, somewhat in the way that we believe the large game of the Tsetse-fly belts harbours the organism of 'Ngana, or as the acclimatised ox will, while apparently healthy, act as a host to a modified form of the red-water organism (Pyrosoma Bigeminum), and be capable of re-establishing the disease in its acute form in a susceptible animal, or, again, as the native child in a malarial district is the host and the means by which the malaria of man is propagated and maintained.

Such a theory would not conflict with the possibility of direct transference from one acutely infected animal to another, which, in the presence of an active agent, such as the mosquito, might develop into an epizootic of magnitude which we should call a "wave of the disease."

It is possible, again, that cases which have recovered from the disease (so-called salted animals), may assume the role of host, and so furnish infective material for the propagation of the disease. Consideration of the well-established fact that the immunity conferred by an attack of the disease is of so short duration and frail character presents a difficulty in this connection.

Several distinct attacks may be produced in the same animal at short intervals, and this with so little diminution in intensity that the disease may prove fatal after an animal has successfully resisted three, or possibly four, previous well-marked artificially induced attacks.

It seems more probable, therefore, that the organism exists in a modified form in some insusceptible intermediary host if the agency of the mosquito in the production of the disease is to be proved to be a correct surmise. Such intermediary host, however, need not necessarily be an insusceptible equine or even a warm blooded animal at all, for we know that frogs, for instance, are capable of harbouring in their circulation forms of parasitic life not dis-similar to the malarial organism, while frogs are inseparable, of course, from the moist and marshy localities we are led by our experience to look upon as so closely connected with the disease in question.

Another fact tending to strengthen this insect theory is practically the outcome of the observation, that if stables are kept filled with pungent smoke during the night the animals so protected remain free from the disease. Use has been made of this fact in stables occasionally, but in an empirical manner when regarded as a precaution against insect attack. The use of smoke (or "smudges") against the attack of the mosquito is well understood in America and elsewhere.

The point of attack is conjectural, as indeed are all these speculations concerning this theory of the cause of horse-sickness. Probably the more thinly covered parts of the body are generally selected. The suggestion that some selective power is exerted by the mosquito causing it to attack the head and thin skin over the eyes—it being here that the first objective symptoms are noticed—is probably without significance, as the same swelling of the orbits is produced when the animal is infected artificially by means of the hypodermic syringe.

Another point of conjecture in this theory of mosquito-produced horse-sickness is the species of insect.

Analogy with the malaria-carrying mosquito points strongly to the various kinds of anopheles, and perhaps to some species of the ordinary mosquito or culex, as being possible bearers of the disease.

Anopheles are known to be present over the horse-sickness districts of South Africa, and it is probable that further investigation would prove their widely spread distribution throughout the whole continent.

This insect, which should be recognised by everyone as a possible menace to health, can be distinguished without much difficulty from the ordinary mosquito, by the existence of spots—or maculae—upon the wings when held up to the light, and also by the insect's peculiar attitude when resting upon any object, for it appears to stand with the proboscis, which is in a line with the body, touching the surface, and the posterior part of the body elevated at a considerable angle by reason of the length of the hind legs. The ordinary mosquito, it will be noticed, rests with its body almost parallel with the surface upon which it alights.

Modern science has within the last few months shown the imperative necessity of keeping all persons suffering from malarial fever closely guarded against the attacks of these anopheles, as it is in this manner chiefly, if not solely, that the disease is communicated to others. This short digression is not entirely inappropriate to the subject under consideration, as I have observed anopheles feeding upon horses, both in health and sickness, and have noticed them abstracting blood from their host until greatly distended.

There remains further, when considering this insect theory of the disease, the possibility of the agency of other than the winged insects—such as members of the arachnidae or ixodiae (spider and tick family)—being concerned. There seems, however, no evidence, direct or indirect to point to the likelihood of an intermediary active agent being found amongst these orders. The whole weight of experience goes to show that the hours between sunset and sunrise are more dangerous than the hours of daylight, that moist or marshy places are particularly to be avoided, that rain is attended by a period of immunity, that enveloping animals with smoke increases their safety, and that the cause of the disease is capable of rapid transmission from place to place. This combination of evidence would seem to negative the agency of such comparative immobile insects as ticks, spiders, etc.

In concluding this brief review of the possible connection between the puncturing insect and South African horse-sickness, the fact should again be recalled that the foregoing theories and suggestions are of a speculative and tentative nature, and put forward rather with the object of tabulating and bringing before our review a *resume* of our experiences rather than with the idea of setting forth a scientific thesis, capable of untying the Gordian knot which has puzzled us so long, and this theory (the Subcutaneous or Insect Agency theory), is put forward with more hesitation, as up to the present, December 1900, it has received no notice at the hands of workers with the disease, except in the instance quoted above, in which insects were acquitted of being concerned in the production of the disease.

(*B.*) *The Respiratory Theory* (or the theory dealing with the probability of a horse becoming infected by inspiring the germ of the disease.)

Here again there is an absence of direct evidence tending to prove the probability of the above theory. That such a mode of entry of the organism is possible is almost conclusively proved by the fact that injections of virus or infective fluids into the windpipe of the horse will produce the disease. To this fact too much weight cannot be attributed, for the writer has also by a similar intra-tracheal inoculation produced red-water in a susceptible ox. This latter result, however, cannot be looked upon as impeaching the evidence—now so thoroughly confirmed—that red-water is produced through the inoculating bite of the specifically infected tick, and not by the entry of the organism through the lungs. The fact, however, of its being possible to produce horse-sickness in this way would seem to point to the possibility of the bronchial or alveolar epithelium or lining, furnishing a point from which infection may take place. That such a possibility exists in a perfectly normal respiratory membrane seems improbable, although in this connection the extremely small size of the causal organism should be borne in mind, passing, as it does, with ease through the finest and closest filters of atomaceous clay.

The fact of the chief characteristic post portem appearances being referred to the lungs may possibly have led past observers to entertain this Respiratory Theory being upon such evidence. Nothing, indeed, in the symptoms during life or appearances after death exists to warrant such a theory untenable.

The striking exudation which takes place into the lungs seems to point to these organs as being generally, but not specifically involved in the progress of the disease.

The theory has been advanced, that the disease is contracted by the direct inhalation of spores of the organism.

One South African investigator holds the view of these spores being transferred by winds at high altitudes and subsequently inhaled upon precipitation. The difficulties in the way of the acceptance of such a theory are obvious. Certain conditions favouring the existence of fungoid growths are, however, also favourable to the outbreak or continuance of the disease.

Mushrooms and other fungi are known to spring into existence and spore during the warm moist nights of summer, and it is therefore conceivable that under such conditions pathogenic spores might be liberated and inhaled by animals grazing, and in this way produce the mycosis of the lung described.

Miscroscopical examination on the writer's part up to the present has, however, failed to establish the probability of such a theory, or satisfy the subsequent difficulties of transmission and indefinite reproduction in an animal host.

The nomenclature of the disease (Oedema Mycosis) suggested recently by a South African investigator is therefore possibly inapt, and certainly premature, inasmuch as other careful workers have failed to demonstrate any degree of mycosis or mould-invasion in tissues after death.

That no inflammatory lesions are discoverable in the lungs would seem to point to a morbid process in which the lung tissue played a passive or mechanical part rather than being the seat of a primary infection and development.

That organisms of small size would be capable of easy dissemination through the air is comprehensible, but it is not so easy to see why such dissemination should take place only at certain times of the day.

Points of resemblance exist between Horsesickness and Rinderpest, notably in the affection of the lachrymal mucous membrane and in gastric symptoms, which would tend to the classification of the former as a catarrhal disease capable probably of dissemination by the air. Practical experience, however, disproves the fact that any great degree of infectivity exists, as animals stabled closely on either side of a case of the disease rarely contract the disease.

Experiment alone will be capable of clearing up this obscure theory as to whether the cause of Horsesickness is due to respiratory infection.

We now pass on to a consideration of the possibility of the disease being due to a cause gaining entrance to the system through the alimentary canal.

This I have termed the Ingestive Theory, from the possibility of infection through the mucous-membrane of the digestive apparatus—when in either a normal or an abnormal condition — including such possible factors as intesttinal parasitism, etc. Infection through the mucous-membrane of the eye will also be considered under this head.

Popular opinion—based upon no scientific grounds, however—inclines to the idea that the disease is "picked up" by an animal when grazing upon food moist with a dew, this materies morbi gaining an entrance to the system through the stomach or intestines.

Such an impression tends to become confirmed upon observing the undoubtedly specific part played by the intestinal tract in the course of the disease. In a very large percentage of the cases which have come under my notice since 1896, an intense gastro-enteritis has been present upon post mortem examination. So marked is this condition often that one is reminded of the post morten appearance of a case of arsenical or other corrosive poisoning. That such a condition should be attributed to the ingestion of any directly acting cause—such as irritant food or direct microbic action—is negatived by the fact that these symptoms are frequent concomitants of the disease produced artificially by means of the hypodermic syringe. As this fact, which I consider of grave importance, seems wholly or in part to have been unnoticed by other workers with this disease, I wish it to be understood that these observations apply to the disease as manifested in Natal.

Such a condition, of course, does not negative the theory that the naturally produced disease may be contracted through the stomach or intestines, but it shows that the specific gastritis cannot be due to the ingestion of any directly acting cause.

The incidence of zymotic diseases upon any particular organ or set of organs, is, of course, well recognised; one recalls the nephritis of scarlatina, the endocarditis of rheumatism, gastritis of rabies, and—perhaps the most striking instance of the selective affinity of disease for specific local manifestation—the lesions of foot-and-mouth disease.

The same obscure selectiveness may account for the intestinal lesions of horsesickness, and furnish an analogy for the presumption that such lesions may exist independently of any cause acting locally upon the mucous-membrane of the stomach or intestine.

That with the ingestion of dew the organism gains an entry into the system seems improbable, for the infective cause indeed can be but seldom existant with or upon the dew, and this is evident, inasmuch as dew must, as an everyday occurrence, be ingested by animals turned out to graze. That such dew entangles or precipitates the organism from a considerable height is improbable, because dew formation takes place only from air-strata in contact, or nearly in contact with the earth. This theory of precipitation from considerable altitudes seems also to be negatived when one considers the freedom from horse-sickness experienced during rain.

The possibility of producing the disease through ingestion of infected material is well demonstrated, though whether such infection is possible in the

presence of a perfectly normal gastric-membrane is still a matter for decision. In this connection we should bear in mind the instructive experiments made of late years, showing that insusceptible animals can be made liable to certain diseases by introducing the organism of the disease into a stomach, the lining membrane of which has been rendered abnormal by the contact of lactic acid or other agent producing a slight degree of gastritis.

The absolutely normal condition of the gastric membrane of the South African horse can but rarely exist where parasitic invasion of the alimentary canal is so universal.

If the stomach were the only medium by which the disease might be introduced some colour might be lent to this theory, but subcutaneous, intravenous, and also intra-tracheal injections prove that the Ingestive Theory cannot be considered as the only theory at present tenable or capable of satisfying all the difficulties of this illusive disease.

Post mortem evidence seems to point to the fact that the stomach lesions produced by ingestion of infective matter are more severe than those induced in the natural course of the disease. In the case of one animal receiving 150 c.c. of virulent blood the reaction was so great as to be described in the post mortem report as "showing the most marked inflammatory changes, patches denuded of epithelium, and small erosions. The colour, instead of showing the usual dark venous colour of gastritis, was of the brightest cherry red." In the duodenum "actual erosions and loss of tissue" are noted. This appearance is produced by the introduction simply of a small quantity of virulent material into the stomach.

Contracting the disease by ingestion would seem to be a manner of infection, presenting none of the theoretical difficulties of the Inoculation Theory, did not one see the ease and certainty with which, under given conditions, this latter infection may be accomplished.

While the possibility of infection being able to take place in this manner is readily understood, the curious intermitting nature of the infecting cause is one of the points requiring elucidation before the Ingestive Theory can be seriously entertained.

The fact of the conjunctiva or mucous-membrane of the eye being constantly inflamed—or "injected," as it is termed—to a degree more or less marked, is probably of small significance in consideration of the aetiology, or cause, of the disease. One recalls the frequency of this symptom in Rinderpest, Rabies, Influenza, Bubonic Plague, etc. That infection through the conjunctival-membrane is possible would seem likely from recorded cases of Glanders, or specific Iritis, contracted in this manner, where contagious matter has gained access to the eye. There seems, theoretically, no reason why such infection should not take place in the case of South African Horse-sickness, although no evidence has yet been brought forward in support of the theory. I have failed to induce the disease by spraying contagious fluids into the conjunctival sac. The fact again of other animals in close contact with an infected horse remaining free from disease would seem to point to its non-volatile nature.

That flies might prove the bearers of the contagion from the eye of one animal to another is possible, but purely conjectural, especially when one considers the habit of the ordinary Muscae, and their quiescence during the time which—by the strongest weight of evidence—is looked upon as dangerous.

To complete the brief survey of this theory there remains the question of the possible agency of Intestinal Parisites, which are probably never absent in the South African herbivora, but are always to be demonstrated by careful inspection of the intestinal contents.

The frequency with which the larvae of the Oestrus Equi (Bots) are noticed in *post-mortem* reports of the disease should not remain unnoticed in view of the manner in which the integrity of the gastric membrane is affected by these parasites. No confirmatory evidence, however, exists in this case upon the probability of such a factor obtaining in the aetiology of South African Horse-sickness.

Upon a consideration of the various theories set forth above, the evidence of the greatest weight seems to lie in favour of what I have termed the Inoculation Theory. All the features of the disease most difficult to understand seem reconcilable to a greater extent with this theory than with the others mentioned. While much work has been done in the past it is probable that still more remains for the future. In my own researches into this disease, meagre as they have been, and hedged about by many limitations, I have sought rather to probe into the darkness surrounding *the cause of the disease* than to devise means for its actual cure or prevention, believing that the quickest way of establishing Prevention upon a sound basis lies primarily in the recognition of Cause. The difficulties in the path of the enquirer are unusually great, seeing that no organism is discoverable, and that the actual mode of production of the disease is equally obscure. Another serious difficulty—appreciable only by those who have undertaken research work of this nature—is that the malady is practically untrans-

ferable to other animals than those of the Equine species. Were it possible to produce the disease in guinea-pigs or rats, or other easily obtained and inexpensive animals, there is no doubt that the investigation of the disease would be rendered much simpler. Horses, on the other hand, are procurable only after difficulty and expense, and I have often remarked that the very economy which it is imperative to observe in conducting an investigation of any extent leads to the use of animals not the most suited (by reason of age and debility) to the enquiry in hand.

As far as I have been able to judge the endeavour directly to prevent or cure the disease has met with so small a measure of success as to be unsatisfactory.

To those who are impatient for practical results I would refer to the labours of such men as Koch and Pasteur, where many years of assiduous labour in solving the problem of obscure diseases met often with but partial success.

Unlimited time, extended knowledge, as well as much expense and thought, are necessary to the undertaking of an investigation into an obscure disease, and the prospects of success in such work are not heightened when able and experienced men of science have already thoroughly exploited the field and found it "stale and unprofitable."

The future, however, for the horse-owner, and particularly for the horse-breeder, is not a hopeless one. Investigators have already accumulated a mass of evidence—much of it negative in nature, but still valuable, about the disease horsesickness, and progress is continually being made towards a successful solution. In conclusion, I would ask the Natal agriculturist to briefly review in his mind the possible causes of the disease whch I have laid before him, and bring to bear upon them his past experience.

Many have already written to me their opinions on the subject, some dissenting but many concurring with my inclination toward the Inoculative Theory, as being the most probable solution of the question. For the present I only feel free to state that my experience and investigations into the cause of the disease have led me to the provisional adoption of this as the most probable theory. As time goes on, the experiments which have induced me to lean towards this opinion will be placed before the public, but, as I said in the earlier part of this article, I consider it a matter for regret that immature and inconclusive results should be placed in the hands of the public before time and extended experience have produced the proofs which alone warrant publicity.

NOTE.—*The foregoing Article was written for the pages of the "Natal Agricultural Journal" during the writer's return passage to South Africa after sick leave in December, 1890. The references to the work of Nocard and M'Fadyean, not then published, were added while revising for press.*—H.W.-P.

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